Among these slightly protracted notes the ch, sch, i, and ä predominated. As to their possible meaning I am still in the dark; I was not even able to discern whether they were to express a sensation of pleasure or comfort, pain, or passion. The animals seemed to be in quite a normal condition. As I shall relate further on, I overheard afterwards a common wall-lizard of Capri, grown blind by conjunctivitis, in the act of producing the same sounds.

After it had been attempted to reject my statements, without any reasoning, indeed by declaring the voice described by me to have been the effect of a rheumatic affection of the mucous membranes, which the Italian lizards had contracted in our cold German climate, I happened to hear the same sounds from a lizard under circumstances wholly excluding every supposition as to its being an abnormal voice. I have shortly communicated the fact in my paper "Untersuchungen über das Varüren der Mauereidechse, ein Beitrag zur Theorie von der Entwicklung aus constitutionellen Ursachen, sowie zum Darwinismus," in Troschel's *Archiv für Naturgeschichte*, xlvii. Jahrgang, 1881, and separately in Nicolai's Library, Berlin, 1881 (pp. 66-68), I quote the following passage:—

"In 1877, having ordered a man to search the middle Faraglione rock for lizards, I waited for his return in a little boat at the foot of the rock. After a while, the man came down with a number of captured lizards tied up in his pocket-handkerchief. I was going to take a specimen of *Lacerta muralis cœrulea—Cœrulescens mihi*—which he had just released, in my own hand, when it uttered repeatedly, in swift succession, a series of very sharp tones, sounding like 'bschi,' and reminding me of the hoarse piping of a mouse or a young bird."

Moreover, I mentioned that Dugès already tells us of *Lacerta Edwardsii*, a little lizard peculiar to the shores of the Mediterranean, that it is apt to utter a sound resembling the creaking of a Cerambyx. And he further reports that *Lacerta ocellata*, a large lizard of the south, when angry, will expel its breath so vehemently that a sort of voice is produced. And M. H. Landois, of Münster, at my request, informed me last year that *Lacerta viridis* was able to utter a distinctly hissing or blowing sound. These reptiles on being approached on a hot summer day, would rush furiously at their enemy, at the same time making use of their voices, so that they were distinctly heard.

Excepting the few instances above mentioned, in which the existence of voices in lizards has been observed, I am not aware of any corroborative evidence preceding that discovery, a circumstance which is easily explained by the general taciturnity of the animal, which but rarely makes use of its voice.

On the other hand, the *Tapoya Douglasii*, a kind of lizard living near the Oregon Lake, when irritated, hisses very audibly. In like manner are the Iguanas reported to hiss and blow on being caught. TH. EIMER,

Professor of Zoology in the University of Tübingen

Sea-shore Alluvion—Langley Point

THIS spit of shingle, thrown up under the lee of Beachy Head and to the eastward of Eastbourne, is formed, like Dungeness, to windward of what was anciently a large tidal estuary forming Pemsey or Pevensey Haven. At the Roman period the mound on which stand the ruins of the castle, was washed by the sea. The windward supply of shingle forming this ness came from the beach at Brighthelmstone, a fortified town below the cliff, in Elizabeth's reign, on the site of the chain pier, gradually undermined by the sea, and not wholly destroyed until the end of the last and commencement of the present century, and the growing out of Langley Point is coincident in time with the destruction of the Brighton beach as its subsequent retreat and decline are coeval with the rapid increase of Dungeness to the leeward. In effect, Langley Point in 1736 projected three-quarters of a mile further into the sea than at present, and it is a curious fact that the breakwater proposed by the Harbour of Refuge Commission of 1840, parallel to, and one mile from the shore in Eastbourne Bay, opposite the "Wish Tower" site and the Grand Redoubt touched at the north-east end of its eastern *kant* the low-water line of 1736, as shown by the surveys of Desmaretz, the well-known ordnance surveyor of that period, but situate in three to four fathoms of water in 1840. This is a striking illustration of the amount of speculation respecting any increased area of anchorage to be obtained and maintained by artificial works in the vicinity of these shingle moles or inclosing recessions therein. In Desmaretz's time the bays west and east of this formation, viz. Eastbourne and Pevensey bays, like those now at Dungeness, must have afforded considerable shelter with three fathoms of water, now, however, reduced to one, and the area of shelter correspondingly curtailed.

Dummer's plan of 1698 shows that then the haven was open up to the castle, with the site of an old outfall about one mile west of the then entrance, which had been deflected eastward by the travelling shingle, and about this period, from its becoming constantly blocked up, the land-owners appear to have taken steps to render the drainage permanent by placing a sluice and trunk at the entrance, so that the haven has lapsed into a marsh sewer or drain.

A reference to well-known maps shows that this Ness must have advanced seaward up to a certain period, at the rate of ten yards per annum, when, however, the western supply became greatly diminished by the old Brighton beach being gradually used up, the subsequent diminution and retreat of this point afforded material for the continued increase of Dungeness to the leeward of it.

From 1724, downwards, the recession of the point has averaged over certain terms a rate of from seven to ten yards per annum, entailing the abandonment of several of the Martello Towers which fringe this portion of the coast, as well as the west fort at the Point, dismantled forty years back, also constant expenditure in heavier and deeper retaining walls in front of the fosse of the circular redoubt, at its western extremity, to check the repeated local encroachment of the sea.

It results from this continued recession of the shore that the works at the circular redoubt form an advanced point. In the early part of this century the shore in front of this work was much more seaward, and in front of particular Martello Towers in Eastbourne Bay it has retreated over certain terms of years at the rate of one yard per annum. This is shown by the known distance from the towers to high-water mark at the time of their construction. The waste since the erection of these towers has been mainly westward in Eastbourne Bay, accompanied by a certain local increase for a short distance to the eastward of the Point in Pevensey Bay, a similar result to that experienced at Dungeness.

From Dungeness to Langley Point, a stretch of thirty miles, except where intersected by harbour mouths, there is an uninterrupted belt of shingle. Over the last century an elongation of the east point (Dungeness) appears to have consumed the western surplus supply, as shown by the corresponding retreat of the western (Langley) point. The intermediate belt has with less fluctuation been driven more landward, showing that a littoral wasting away from wave action at one point is balanced by a corresponding increase at another.

The plan by Grenville Collins, 1693, shows Pemsey Haven clearly defined with two arms or branches, and a considerable entrance, but contains no notice of such a projection as Langley Point. The topographical survey by Yeakel and Gardner, 1778, a well-executed map to a scale of two inches to the mile, shows it stretching one and a quarter mile into the sea in a south-easterly direction. Of course the accuracy of the rates of progression and retrocession given above are based on a comparison of Desmaretz's surveys with those of recent date, and depend on the character of the former. The remarkable changes in the coast-line along Eastbourne Bay, its small depth, the little protection afforded by Beachy Head, and the eastward movement of Langley Point, are, as in the Dungeness case, arguments against artificial works in either of these bays.

J. B. REDMAN

6, Queen Anne's Gate, Westminster, S.W., April 29

Colour Perception

MR. HANNAY'S explanation of the colours observed in his dark rooms, seems quite in accordance with orthodox science. It is not the explanation I should myself offer, but as that would occupy too much space, and as I am conscious I should not carry the public with me, I refrain from entering on it.

What I do object to is the notion apparently entertained by Mr. Hannay, that his attempted explanation of this single phenomenon, explains also the experiments in the formation of colour I showed him. How can this explain the fact that I can show in the space of a few inches, from mixture of black and white alom, a dozen different colours side by side, mostly as clear and bright as if painted? And how does it explain the fact, that using the exact same proportion of black and white,

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and working at the same speed, the motion to the one hand will produce *red*, and when reversed, *blue?* Mr. Hannay also seems to imply that the colours of my experiments to be seen well, should be looked at passively or without keen attention. On the contrary, the more light thrown on them, and the keener, fresher, and younger the eyes of the observer, the more brilliant are the colours, and if a boy of ten or twelve years old, who never saw anything of the sort before, be called in, he will describe them better than grown people.

Scientific men have hitherto considered it a sufficient explanation of these experiments to say the effects are "physiological," as if colour were ever anything else. Newton says, speaking of *coloured rays*, that he uses the term merely to suit the understanding of the vulgar, as they are nothing but a certain power and disposition to stir up a sensation of this or that colour. Prof. Ogden Rood again classes these as *subjective* colours, a word which, if it has any meaning at all beyond a very limited one, can have none with reference to colours which remain permanent so long as the machine is in motion.

27, York Place, Manchester, April 22 NAPIER SMITH

How may Clouds consisting of Liquid or of Frozen Water be Distinguished?

IN NATURE, vol. xxv. p. 529, M. de Fonvielle asks my opinion as to what observations may be made in a balloon to discover whether in a cloud whose temperature is below zero the minute particles of water are liquid or solid.

There may be difficulties in the way of deciding by direct observation of the form of the particles, whether they are globules or crystals. But since H. B. de Saussure, a century ago ("Essai sur l'Hygrométrie), by means of a single lens, could distinguish in the air over heated water, globules of condensed water of different size, greater ones which appeared to him full, and smaller ones which he regarded as hollow; and when in more recent times A. Waller (*Philosoph. Transactions*, 1847) could make his "microscopic observations on the so-called vesicular vapour of water as existing in the vapours of steam and in clouds, &c.," with the result that he denied the existence of mist-vesicles, it seems possible that by means of a magnifying instrument the form of the particles suspended in the air can be recognised.

A sure evidence, but obtainable only under favourable circumstances, that the particles forming a cloud are ice-crystals would be the observation of the halos of 22° and 46° radius and of parahelions as produced by the cloud. HERMANN KOPP

Heidelberg, May 3

On the Conservation of Solar Energy

DR. SIEMENS's theory of the constitution of the sun implies that there is an absorption of solar rays constantly going on in space. If this is so, space cannot be perfectly transparent.

An astronomer of the early part of the present century—I think it was Olbers—came to the same conclusion, though from different reasons. He found that as the space-penetrating powers of the telescope is increased, the number of stars that become visible does not increase so rapidly as it would if they were evenly scattered through space, and if space were perfectly transparent; and he concluded that most probably space is not perfectly transparent. This, however, is by no means conclusive, because it is possible that the reason why the number of stars that become visible does not increase as it ought to do on the supposition, is that the number of stars in the universe is limited.

JOSEPH JOHN MURPHY

Old Forge Dunmurry, co. Antrim, May 3

CYCLONES[1]

II.

IN our former article we dwelt on the deductions arrived at by the author from a consideration of the mechanical theory of cyclones. We will now proceed to examine how far such theoretical relations are corroborated by the results of observation. The results of observation utilised by the author comprise those of the Rev. W Clement Ley, published in his "Laws of the Winds"; those of Prof. Loomis, deduced from a study of the U.S. Signal Service charts; those of Dr. Hildebrandsson, with regard to the upper currents from an examination of the Danish synoptic charts; those of Capt. Toynbee, from a study of the Atlantic storms; and lastly, some contained in a recent work on the hurricanes of the Antilles, by Padre Viñes of Habana. Mr. Ferrel at the outset pointedly remarks that for a mariner to be able to make use of the laws deduced from a study of the theory of cyclones, not only a knowledge of such laws is requisite, "but likewise of the *normal* states of the wind and of the barometric pressure in all parts of the ocean and at all seasons of the year, unaffected by the abnormal disturbances of these progressive cyclones; since with a knowledge of the normal conditions of the winds and of the barometric pressure at any time and place he can perceive the first indications of the abnormal disturbances which are the forerunners of these storms, and so can be on his guard, and then with a knowledge of these storms or cyclones, he can generally avoid at least their most dangerous part."

With regard to the first result from theory, viz. the general incurvature of the winds in a cyclone, which was formerly altogether denied by the cyclonists—so-called—Reid and Piddington (not Redfield), or inordinately magnified in every case by Espy, and other upholders of the radial theory, there seems to be no doubt from the results of observation here given, as well as from others not cited by the author, that the wind deviates to a considerable extent from the tangent to the isobars inwards towards the low centre. Moreover, in accordance with theory, this inclination is greater at inland stations where there is more surface friction than at or near the sea when it is less. Thus Ley found the inclination to be about 29° for inland stations, but only 13° for those on the coast. This difference between the inclination at sea and on land may perhaps account for the tenacity with which sea captains still cling to the notion that the wind blows in circles coincident with the isobars; since it is precisely at sea where the incurvature should theoretically be least. The increase in the inclination corresponding to a decrease in the latitude is likewise borne out by observation. Thus from Ley's observations, which embrace North and South Europe, the mean inclination to the isobar is about 25°, from those of Capt. Toynbee on latitude 50° it is 29°, from those of Loomis nearer the equator in America 47°,[1] from those of Padre Viñes in the Antilles 45°, and we may add from some in the Bay of Bengal, mentioned by Blanford, about 42°.

It is thus pretty evident, as the author remarks, that "though the horn-cards of Piddington based on the strictly circular theory of the winds may still be used at sea in high latitudes without great error, yet nearer the equator they must become more erroneous, and entirely fail at the equator if cyclones could exist there." Mr. Meldrum, of Mauritius, as far back as 1867 drew attention to the disasters which resulted in consequence of vessels having estimated the direction of the centres of cyclones according to the rules of the circular theory, and since the publication by M. Faye of his "Défense de la loi de tempêtes," has strenuously opposed the resuscitation of this exploded doctrine. He has also lately given an admirable proof of the truth of the incurvature, by publicly announcing when the wind, in a cyclone on March 21, 1878, was from the north-east in Mauritius, that the centre of the storm was not to the north-westward according to the circular theory, but to the west-south-westward, which was afterwards found to have been the case.

It is a manifest duty therefore which mankind owes itself, if the dangers of the sea are to be minimised, that the amount of inclination of the wind to the isobar should be determined by observation in different seas and for

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[1] "Methods and Results of Meteorological Researches for the use of the Coast Pilot." Part II.—On Cyclones, Waterspouts, and Tornadoes. By William Ferrel. (Washington, 1880.) Continued from p. 12.

[1] Some of these included very gentle winds.

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